founded on false data, we trust it could never become a temptation to our present author.

But besides the difficulty of writing on electrical measurement without mathematics, there is, in our opinion, an attempt to catalogue and describe far too great a number of instruments and methods. In making this criticism, we cannot support it by mentioning names; but the author knows well, and everyone else knows, that many of the instruments and methods to which space is devoted are absolutely worthless; and it would be infinitely better to omit them, and thus both avoid confusion and save space, which might well be given to those that are of importance.

Altogether, the book requires re-writing, by which it could undoubtedly be made of very considerable value. The style is not good. With a sort of self-consciousness, Mr. Swinburne calls himself "the writer" throughout. Some of the criticisms—for example, that on the "B. A. Committee" (p. 22), and a remark on one of our most highly-valued scientific men (p. 110)—are altogether out of taste, coming from the pen of one who has his reputation still to win.

OUR BOOK SHELF.

Galileo and his Judges. By F. R. Wegg-Prosser. (London: Chapman and Hall, 1889.)

THIS work is a temperate discussion of the vexed question of the treatment of Galileo by the Pope and the Congregation of the Inquisition. The facts are not new; Mr. Prosser puts himself unreservedly in the hands of M. Henri de l'Epinois, whose article in *La Revue des Questions Historiques* is well known, and who has, Mr. Prosser says, gone to the trouble of consulting at first hand all the documents that could be found at the Vatican bearing on the subject. Mr. Prosser, in drawing his conclusions from the facts, adopts a kind of middle ground. He is a Catholic, and though he is too sensible a man to follow many of the Catholic writers in their conclusions, yet he seems to be shocked at the standpoint taken by a few of the Catholic writers who have condemned the treatment of Galileo. Thus he occupies a position between keen controversialists like the late Dr. Ward, on the one hand, who hold that not only did the Congregations act within their rights and their legitimate sphere, but that, looked at from the point of view of the early part of the seventeenth century, they acted wisely and prudently, and Catholic writers like Dr. Mivart, on the other hand, who assert (these are Mr. Prosser's words) "that the Church has no authority to interfere in matters relating to physical science, and that the issue in the Galileo case has proved the fallacy of her attempting to do so; that without entering into the discussion of what ought or what ought not to have been done in former times, we of the present generation have evidence sufficient to show us that scientific investigations should by right be free from the control of ecclesiastical authority." The first step taken by the Church against Galileo was in 1616, in censuring him for his teaching, and warning him of the consequences if he continued to teach the doctrines, first, that the sun was the centre of the universe, and therefore locally immovable; second, that the earth was not the centre of the world, and moved round itself diurnally. The first doctrine was declared by the Qualifiers—that is, the committee appointed from the Congregation of the Inquisition—to be foolish and absurd from a philosophical point of view, and heretical since it contradicted the meaning which had been given to certain passages of Scripture by the Church. Galileo promised to obey the warning, "*ut supra dictam opinionem . . . omnino relinquat, nec eam de cetero quovis modo doceat teneat aut defendat verbo aut scriptis.*" Mr. Prosser enters into a very long argument to show that this decree of 1616, though founded on reasons of doctrine, was merely disciplinary, and not given on a matter *de fide*, in which he is now and then rather casuistical. Galileo after this remained in peace till he was summoned to Rome to answer for the printing and publishing of his "Dialogue" in 1632. The heads of accusation are set out at length in the present work, but substantially they come to this, that he had disobeyed the order of 1616, and had continued to teach the same doctrines as those for which he was then reprimanded. It is impossible not to see that in summoning him to Rome the Pope was to some extent actuated by feelings of pique, for the fool of the "Dialogue," Simplicio, undoubtedly represents His Holiness. Mr. Prosser goes on to show that, having regard to the state of knowledge at the time, the Inquisition could have done nothing else but convict Galileo. The defence of the latter was threefold. In the first place, he said that Bellarmine had informed him that he might hold the Copernican doctrine as an hypothesis. This was undoubtedly the case; but it appears as something more than an hypothesis in the "Dialogue." Galileo answers to this that he had merely put the theory in the mouth of a speaker whose teachings were combated by the other speakers. Secondly, he maintained that he had not contravened the order given to him not to teach or expound that abominable doctrine in any way. This is hardly correct, as the "Dialogue" will show. Thirdly, he declared that he did not remember having been forbidden to teach it. But he could hardly have forgotten the terms of the order of 1616, which have been quoted above, nor the rebuke given him by Bellarmine by order of the Pope.

Observations on the Embryology of Insects and Arachnids. By Adam Todd Bruce, B.A. of Princeton College, Ph.D. of Johns Hopkins University. A Memorial Volume. (1887.)

THE subject-matter of this volume formed the thesis submitted by the author when he presented himself for the degree of Ph.D. at the Johns Hopkins University. After his lamented death, in 1887, the thesis was reprinted, exactly as he wrote it, as a memorial volume. He had made many additions to the work which is here recorded, but as the notes were unaccompanied by drawings it was impossible to make use of them. An account of the life and scientific work of the author is written by Prof. W. K. Brooks. The early death of Dr. Bruce, at the age of twenty-seven, prevented any very extensive amount of scientific research. It will, however, be clear to any reader of the careful and excellent work contained in this paper, that American biological science has lost an investigator of the very highest promise. Dr. Bruce had also very carefully studied, in conjunction with Prof. Brooks, the early stages of the development of Limulus, and it is much to be hoped that these results may be published at no distant date. A thorough study of the earliest stages of this most interesting form by so careful an embryologist would be extremely valuable. Prof. Brooks informs us that the work included "the segmentation of the egg, the formation of the blastoderm and of the germ-layers, and the anatomy of the young larva . . . illustrated by nearly a hundred drawings." I mention this in the hope that some means of publication may be found in this country, if the claims upon the space of the *American Journal of Morphology* are too great to admit of the appearance of a paper on so important a subject in what appears to be its appropriate position.

The volume contains an attempt to settle the most difficult questions concerning the earlier stages of the development of spiders, Lepidoptera, Coleoptera, and Orthoptera, while less complete observations were made

© 1889 Nature Publishing Group

upon Neuroptera and Diptera. Among insects, the Lepidoptera were studied with especial care, the type selected being *Thyridopteryx ephemeraiformis*. The careful account of this embryology, together with the numerous excellent figures, entirely substantiate the author's claim that the study of this type, "if it has brought to light nothing new, has, in the opinion of the writer at least, settled some important points connected with the embryology of this group of insects." In the account of segmentation and the formation of the blastoderm, the author completely confirms Bobretzky's descriptions. The development of *Thyridopteryx* occupies twelve quarto pages: for the details the original must be consulted. The account of the embryology of Orthoptera, represented by *Mantis* and the grasshopper, and of the embryology of spiders, is also very complete.

At the end of the paper many interesting and suggestive conclusions are appended. Among these it is significant that a writer who has done so much work upon the early stages of Limulus should unhesitatingly regard this latter form as an Arachnid. The Trilobites he considers as "possibly the ancestral form of Limulus."

Only a short account of this excellent paper has been given here. All those interested in embryology, and the light shed by it upon morphological science, will, of course, make a careful study of this work. E. B. P.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Satellite of Procyon.

IT is well-known that Procyon, like Sirius, does not travel through space in a straight line, its motion undergoing changes similar to those which would result from the disturbing action of a large satellite. This explanation was advanced by Bessel in 1844, and though the satellite has not yet been *seen*, its existence has been placed beyond reasonable doubt by Dr. Auwers's investigations on the subject.

Does it not seem probable that this interesting body may be revealed by the aid of photography? It is now possible to photograph stars and nebulæ which are beyond the visual range of the most powerful telescopes; and if the companion of Procyon, like that of Sirius, be self-luminous, there would seem to be a good prospect of obtaining its image on the sensitive plate.

As the companion is doubtless in pretty close proximity to its brilliant primary, it will be necessary, for photographic purposes, to intercept the image of the latter by means of a suitable screen. Since the *direction* of the satellite at any time can be found from Dr. Auwers's elements,[1] there would be no uncertainty as to the position in which this eclipsing disk (or wire) ought to be placed, though its proper adjustment would be a somewhat delicate operation. Should the satellite be photographed, its position will become known from its configuration with respect to other stars recorded on the negative.

If a very large telescope were employed, the images of both components, as distinct and separate dots, might be obtained on the plate. For Procyon, a very short exposure would be requisite, and this could be secured by the use of a movable stop or screen, similar to that devised by Prof. Pritchard, and used for parallax work at the Oxford University Observatory.

It is scarcely necessary to dilate upon the interest which would attach to a photograph showing Procyon's companion. As the parallax of Procyon has been satisfactorily determined by Dr. Elkin and others—being 0″·266 according to the Yale College observations—we could ascertain the *actual* as well as the *relative* masses of the two components. And the brightness, or more strictly speaking the photographic magnitude, of the satellite might also be determined with some precision.

It may not be too late to obtain such results during the present season, but exposures of four or five hours, under good conditions, will not be practicable before next winter.

J. M. BARR.

St. Catharine's, Ontario, Canada, March 4.

"Les Tremblements de Terre."

THE issue of NATURE for February 7 (p. 337) contains a review of the little work on earthquakes published for me by Messrs. J. B. Baillière. The anonymous author of the article makes several criticisms on my book to which I desire to reply.

Your critic thinks it a grave fault not to have entered into a detailed description of the seismographs and seismometers at present in use. He reproaches me in particular for having but just mentioned Prof. Ewing's duplex pendulum seismograph; for having omitted to speak of the same inventor's horizontal pendulum seismograph; and especially for seeming to ignore the experiments made with these instruments by Prof. Ewing in Japan. I confess that I had not been struck by the excellence of the instruments in question, and that it was not through an oversight that I omitted to describe Prof. Ewing's observations in Japan, while I quoted in detail those of his *confrères*, Messrs. Milne and Gray, in the same country.

Your critic defends with some acerbity a certain class of seismographs, and wrongly accuses me of failing to appreciate the principles on which their construction is based. The objections which he makes to my treatment of M. Cavalleri's pendulums of unequal length are entirely refuted from a theoretical point of view by the learned note due to M. Poincaré, which is inserted on p. 46 of my book. I need not insist further on this point.

Your critic thinks I have not done sufficient justice to the work of the Italian *savants*: he forgets the limits necessarily imposed on a book destined especially to give to the general public an idea of the present state of an important question.

The writer of the article regards the seismographs of to-day as perfectly sufficient for all scientific needs. I am far from being the only person engaged in the study of earthquakes who does not share this opinion. Finally, he describes, and not very clearly, the experiments which I made with M. Michel Lévy to measure the rate of propagation of disturbances through the soil, and the registering apparatus designed for this purpose. According to the writer, these experiments constitute the only advances we have made in the study of earthquakes. If he had rendered justice to our work on the subject, we should have been content, and I should have raised no objections to his article. But your critic reproaches us with having given results which are masked by inevitable causes of error. A more careful study of the book, and especially of the extracts from our original memoir, published in the *Comptes rendus* for 1885 and 1886, would have preserved him from so inexact an assertion. In fact, he unjustifiably mixes up the *preliminary* experiments, made at Le Creusot by means of an apparatus exactly similar to that used by Mallet and Abbott,[1] with what really constitutes the basis of our work—I mean the determinations made with the aid of photographic registration and explosives. What your critic calls the "personal equation" of the instrument is here nearly negligible[2]; and the merit of our method lies precisely in this point, which distinguishes us from our predecessors.

We would beg the readers of NATURE to verify for themselves the truth of our statement. This verification will enable them to judge of the value of the article laid before them.

F. FOUQUE.

Finding Factors.

IT may add interest to Mr. Busk's ingenious method of distinguishing between prime and composite numbers to state the algebraic basis on which it rests.

Let N be any number, and n^2 the next higher square number, and let $N = n^2 - r_0 = (n + 1)^2 - r_1 = (n + 2)^2 - r_2 =$ &c. r_1, r_2 are formed successively from r_0 by the successive additions of $2n + 1$, $2n + 3$, . . . the increments being in arithmetic progression, so that $r_m = r_0 + 2mn + m^2$. As soon as r_m becomes a square, N is expressed as the difference of two squares, and its factors are found.

---

[1] At present the position-angle of the satellite is *about* 233°. Its *distance* probably amounts to but a few seconds of arc, and may be within 2″.

[1] See p. 219 *et seq.*

[2] The only subsisting cause of error is that due to the inertia of the mercury, which we have estimated and introduced into all our calculations (see p. 246).

© 1889 Nature Publishing Group